parts. The fimbriated extremities remain open, unlike other inflammations, until the disease is quite advanced; this is due to the slow advance of the disease, the mucous membrane becoming so thickened that it is thrown into a somewhat spiral formation, these spirals then adhere to each other, thus obliterating the lumen. In other inflammations the progress is more rapid, going out at the fimbrinated extremity, sealing it and converting the tube into a closed sac. The second form is as in other inflammations; the tube is converted into a closed sac sometimes as large as an orange, the contents being cheesy masses. Mallory found tubercular tubes weighing two pounds. The tube may rupture into the bladder or rectum or general peritoneal cavity; the only difference between this and the other forms clinically is the slow onset and more chronic form of symptoms, the temperature, etc., being lower and irregular, as in most T. B. infections. The second or cheesy form is very probably a hematogenous infection.

THE UTERUS.

T. B. of this organ occurs oftener than is noted. It is usually associated with tubal T. B. but may be involved alone and cases have been reported where endometrial scrapings showed presence of T. B.—after that the patient entirely recovered from the trouble.

All so-called catarrhal discharges of persistent nature when originating in the uterus should be carefully watched; a guinea pig inoculation is at least easy to perform if a currettage is not obtainable. Histologic matter is sometimes hard to get. The uterus as in the tubes has the second variety showing like changes, the whole organ being infiltrated with infection, the cervix often escaping, the entire uterine cavity is filled with cheesy masses. This enlargement with lack of menstruation has been mistaken for pregnancy. Sometimes ulcerations occur on the cervix and vaginal walls which look very much like lues; another form in which a cauliflower-like mass which is a mass of T. B. tissue, bleeding easily, has been mistaken for carcinoma. The microscope easily furnishes a means for differentiation.

Primary infection of the vagina and vulva are rare but do occur—the mucous membrane and squamous epithelium giving excellent protection. The vaginal ulcer is a flat ulcer with indurated edge slowly extending.

T. B. ulceration, primarily of the vulva, is very rare but Rieck and Viatte report several cases; the ulcers tend to form fistulae, extending up into the vagina; the process presents peculiar clinical features not unlike lues, elephantiasis or rodent ulcer—the microscope again giving the differentiation. The ovaries have never been known to have primary T. B. but of course are involved secondarily.

In prophylaxis—the care as well as the treatment is obvious—I could not find one authenticated report where a tubercular epididemitis was a cause—but there is a possibility. General cleanliness and care in labor, and also greater care in examinations and operations in the vaginal tract being very necessary, is also obvious.

PERSONAL EXPERIENCE WITH CHOLECYSTECTOMY.*

By LEWIS W. ALLEN, M. D., San Francisco.

In responding to the call for a paper before the society, it is my intention this evening to place before you my experiences in gall bladder surgery and my conclusions deduced therefrom. I will not attempt any historical review of the subject as that can be found fully developed elsewhere. So also with the etiology, symptomatology and diagnosis—fascinating as each of these subjects is, it is contrary to the purposes of this paper to enter into their discussion.

Considering therefore that a diagnosis of cholecystitis has been made there arises in the mind of every surgeon several questions:

Which method of treatment will relieve the patient?

Which method will *cure* the disease, as at that time developed, with the least possible risk to the patient?

Which method will not only cure the disease then present, but also give the greatest assurance of *permanent* relief from recurrent attacks as well as from future complications?

Being of a conservative nature, it is my practice to attempt to relieve the symptoms, rather than rush to operation, all cases in the early stages of cholecystitis in which such symptoms show no systemic infection, or at most very mild infective processes. For I have found in some few cases of cholecystitis, as in some cases of appendicitis, with the subsidence of the attack, the administration of liberal doses of olive oil, an increased care in the diet, both as to its character, quantity and time of ingestion, so as to properly meet the needs of the individual, together with the supervision of the bowel elimination, that in relieving the symptoms the disease to all intents and purposes has been cured. The attacks have not recurred and the patient has remained well.

But where the attacks recur with accompanying gastric disturbances, or where the local inflammation progresses to a more and more serious systemic infection, operation is the only method of procedure. There has been a great deal written upon calcareous and non-calcareous cholecystitis, but clinically in determining upon the necessity or non-necessity of an operation it makes little difference whether there are stones present or not. To be sure when a first attack is subsiding and the pain has been distinctly of colicky type we may prophesy, within our own mind, the probability of recurrence. But sometimes it does not recur, and on the other hand some cases of large stones in the cystic duct or within the gall bladder do not give a typical gall bladder colic as some cases of mucous plugs in, or angulation of the cystic duct, do give a typical colic. Therefore, I repeat that, clinically in determining the necessity of an operation, we are only concerned with the *recurrence of the attacks* in the sub-acute and chronic cases, and in the acute cases, with the *progress* of the infection as exhibited in the increase of the local symptoms, the pulse, the temperature, the leukocytosis,

*Read before the San Francisco County Medical Society, January 12, 1915.

the dry tongue, the onset of chills, showing a general systemic invasion.

Having decided that an operation is necessary, the choice between a cholecystostomy and a cholecystectomy should, generally speaking, be determined only after entering the abdomen. The judgment and experience of each surgeon will necessarily influence his choice in each individual case in answering the second and third questions as above stated. He must ask himself not only which method will cure the disease as at that time developed, with the least possible risk to the patient, but also which will give the greatest assurance of permanent relief from recurrent attacks as well as from future complications?

Before discussing the arguments for or against the one or the other method of operation, let us first classify the various types of gall bladder disease which may be found on opening the abdomen. Clinically they all fall within the general classification of acute and chronic cholecystitis. In the acute types we have the catarrhal, empyematous and gangrenous, with the possibility of the grave complications of cholangitis and general septic infection, and the lesser ones of pericystic abscesses and pancreatitis. In the chronic types are found all the various changes in the walls of the gall bladder and its relations with the surrounding viscera from moderate thickening of its walls to complete obliteration of the cystic cavity; from stenosis of the cystic duct to complete occlusion; from pericystic adhesions to firm attachment to or ulceration into the neighboring organs.

Trouble in the common duct must necessarily be considered as a separate surgical entity which will influence the surgeon's judgment according as to what condition is found. For simplicity's sake I will omit the discussion of common duct disease in this paper.

Although I am a firm believer in conservative surgery, I come before you tonight as an advocate of the more radical operation of cholecystectomy. My first strong leaning toward the more radical operation came just thirteen years ago, in January 1902, after a cholecystostomy for gangrenous cholecystitis and the autopsy findings following the death of the patient on the eleventh day. As the years passed and I observed the swing of the pendulum from cholecystectomies to cholecystostomies, I have carefully analyzed the arguments of the eminent surgeons in favor of the less radical operation but they were not sufficient, in the light of my own experience, to cause me to change. Many of these arguments for and against cholecystostomy and cholecystectomy can best be impressed by illustrative types while others will be summed up later.

I would not have you understand that I never do a cholecystostomy; that there are no cases cured and relieved, beyond a fairly reasonable doubt of future complications by the less radical operation. Such cases, for instance, are the catarrhal ones operated upon fairly early either for stones or mucous plugs in which the inflammation or obstruction has not been of sufficiently long standing to produce marked changes in the cystic wall, or in the attachment of the wall to the surrounding viscera. It has been my experience, however, that patients seldom submit themselves to operation in these mild cases. Many of these that come under observation are relieved by dietary measures, especially those of the mucous type.

Coming next to the empyematous and gangrenous cases I believe they should be considered together. Not that there are not some purely empyematous cholecystitis and others that are distinctly gangrenous but that many are *both* empyematous and gangrenous. It is often impossible to tell even with the abdomen opened and all previous data in mind, although being sure of an empyema, whether gangrene has developed in the walls or not. The above mentioned case is a sad illustration of this condition. Mr. R—, a physically robust man, in the prime of life, with no previous history of gall bladder disease, was suddenly seized with acute pain in the gall bladder region, followed by such signs of collapse that he was sent the same day to the hospital and operated upon within twenty-four hours of the first pains. The gall bladder was drained as well as an abscess between it and the liver. He was relieved, but the signs of sepsis continued until his death on the eleventh day. Autopsy showed over forty ulcerations of the mucosa with areas of gangrene extending from many of these ulcers into the middle layer one-half inch to one inch in circumference. None of these gangrenous areas were visible upon the surface of the gall bladder. Between the gall bladder and the liver, about one and one-half inches from the end of the abscess opened at the time of operation another ulcer had perforated and pus was found extending in a thick creamy layer around on to the under surface of the left lobe of the liver. A stone the size of a pea had produced the obstruction. Observation at the time of operation could not detect this intermural gangrene, or the perforating ulcer between the gall bladder and the liver far back toward the cystic duct. Drainage was entirely insufficient. Since then I have had several similar cases of multiple ulcerations on the mucosa which, when examined after removal revealed sloughing areas beneath. They have all promptly recovered following a cholecystectomy. Temperatures of 104° and 105° have dropped immediately to 99° and 100° with perhaps a slight rise for a day or two. Pulses of 110 and 120 have dropped to 90, and then lower. The subsidence of the symptoms has always been commensurate with the length of time of the infection. Early radical operation in these severe cases has always been followed in my experience with an equally quick recovery. It has been my fortunate experience to have had no deaths following cholecystectomy. Now, in the swing of the pendulum from cholecystectomy to cholecystostomy some nine or ten years ago most of our writers advocated the drainage operation for empyemas. One reason that I was not convinced that this was the operation of choice was because of my experience in such cases as above illustrated. You can not always be positive that gangrene is not present,

or imminent. If present or prospective, drainage is not sufficient.

On the other hand, take a simple empyema case. Drainage may cure the attack then present and the gall bladder may recover to nearly normal. But is it ever normal? And if it is normal in some cases, is it normal in all? With the numerous folds and trabeculae often present can drainage be implicitly depended upon for removal of all foci of later infection? I am convinced that it cannot. Can it assure us a guarantee against holding some nidus of systemic infection? I am convinced that it cannot. Can we convince ourselves as we palpate a drained gall bladder that no damage has been done to the submucosa sufficient to produce later a stricture of the cystic duct, or at the neck of the gall bladder, even in the presence of the free flow of bile, which will give future trouble? I am convinced that we cannot. In 1905 I presented before this society just such a case. A cholecystostomy had been done for cholelithiasis some months previous. A permanent biliary fistula resulted. There was no colic, no mucus, no common duct symptoms. The stools were normal color. The diagnosis of stricture at the neck of the gall bladder was confirmed by cholecystectomy. A counterpart of this gall bladder with stones intact, one in Hartmann's pouch with a firm stricture and a one-eighth inch lumen leading into the gall bladder, was present in one of my cholecystectomy cases. The former after draining for months needed a second operation; the latter was cured and out of the hospital in thirteen days. The point I wish to make is that it is often impossible by palpation and inspection to determine the amount of damage done, or in what condition the gall bladder tract will be left after repair.

For the out and out gangrenous cases most surgeons have advised cholecystectomy. There have been a few, however, who have claimed better results from drainage even in these cases. Such cases would have to be in extremis indeed before I could be satisfied with drainage, and then only with the expectation of future removal in the event of the very improbable chance of recovery. For in such the gall bladder is left in a badly damaged condition.

In the chronic types of cholecystitis both calcareous and noncalcareous, I am an advocate of cholecystectomy unless one can be positive that the biliary tract is unimpaired and will become normal through drainage. In my opinion such assurance can be held with regard to but few of those cases coming for operation after months or years of trouble.

I admit the choice between cholecystostomy and cholecystectomy in many doubtful cases will depend upon the judgment of the operator as evolved from his experiences and the importance which he gives to certain reasons for and against the one or the other operation. Let us then consider some of the arguments set forth by advocates of cholecystostomy.

First, less danger. I believe that this statement had more weight five years ago than now. Even then it applied only to the old or feeble in whom the margin of safety was very small because in my experience the shock of cholecystectomy is little more than cholecystostomy. The chief shock—the opening of the abdomen—is the same in both. The time of exposure is little different if one goes right at the business of removal. In the bad empyematous and gangrenous cases this is more than counterbalanced by the removal of the large area of septic absorption. Now, however, since it is possible to remove the gall bladder after the injection of morphine and hyoscine by local anesthesia, the margin of safety even in the aged is practically the same. Six years ago I operated on a patient seventy-four years old suffering from suppurative cholecystitis by this method. The omentum was found walling off an abscess containing stones outside the gall bladder. Stones and pus were also removed from the gall bladder, but still no bile flowed, and feeling that in this case I must be sure of immediate biliary drainage I went on and removed the gall bladder. She awakened the next morning and wanted to know when we were going to do the operation. With the more recent introduction of anoci-anesthesia the difference in the shock in the two methods is practically nil.

Second, because it is nature's provision. Because it has important functions, first as a safety valve of the biliary system—acting as the vis-a-tergo of the bile. There is no proof that this is a necessary function except the fact that a compensatory pouch has been found at the site of the amputated cystic duct. This can equally well be taken as evidence that nature is able to renew this function. Even the presence of such a pouch is not proof that such a pouch is necessary to health, but only, that given certain conditions, it will form. Against this argument is the fact that patients who have had their gall bladder removed have remained in perfect health for years. I have yet to experience the fact of a patient upon whom I have performed a cholecystectomy even returning to my office complaining of digestive disturbances. Then again it is asserted that the gall bladder is needed for drainage of the biliary passages. To this I would respond that they can be drained as long as desirable through the stump of the cystic duct, and also with the gall bladder removed, one large factor in the need for such drainage has been removed.

Third, that it is needed for drainage of the pancreas to assist the cure of an accompanying pancreatitis. This statement I have been very much interested in. I have had the biliary discharge from drainage cases examined again and again for evidence of pancreatic secretion. In not a single case has any evidence of pancreatic secretion been found in such fluids. Nor does the skin ever exhibit any signs of excoriations as are found about a pancreatic fistula. With the removal of the gall bladder as the chief focus of infection pancreatitis subsides as quickly as, or even quicker than after cholecystostomy. The other argument that a diseased gall bladder should be saved for the possible treatment of a future pancreatitis is an imposition on one's common sense.

Fourth, gall stones may form later in the common and hepatic ducts. Fortunately this oc-

currence is rare. As W. J. Mayo[1] observes, "I have never seen hepatic duct stones without evidence that the original disease had its source in the infection of the gall bladder or common duct, and the common duct infection was almost always secondary to the gall bladder disease." With the exception of those cases, usually giving histories of common duct obstruction, in which a stone or stones have escaped detection, the removal of the gall bladder as a source of future infection would practically eliminate the formation of such stones.

Fifth, the possible use of the gall bladder in case of chronic and irremediable obstruction of the common duct. This fortunately is rare and there can be no arguments as to the advantage of having a gall bladder present for anastamosis. Yet the operation without the gall bladder though difficult is not impossible. On the other hand, against this rare dilemma, must be placed the many dangers, some very grave, of having a diseased organ intact.

Some of the reasons for the radical operation may be enumerated as follows:

First, fistula, mucous or biliary, either persisting from the time of the cholecystostomy, or developing later, sometimes after a period of two or three years. Although this condition in no way endangers the life of the patient yet it is very annoying both to the patient and to the surgeon. Especially here in the west where most patients must pay at least their hospital expenses it is oftentimes a matter of serious financial embarrassment. Of course this point should not be considered if the life of the patient would be jeopardized by the more radical operation. As I stated above, however, it has been my experience that very few cases indeed would be so jeopardized. The more rapid recovery, the more permanent enjoyment of good health following cholecystectomy has confirmed my early belief in the more radical operation. In favorable cases, especially in the acute cases, the sinus may be allowed to close in five days or a week, and the patient leave the hospital in two weeks all healed.

Second, the infected gall bladder as a nidus of future infection, either locally or systemic. It is claimed, and with truth I believe, that it is almost impossible to thoroughly sterilize all the crypts and folds of a diseased and thickened gall bladder wall. This, of course, depends upon the amount of change that has taken place, but granting that it is thoroughly healed, what guarantee is there that it will not later be reinfected? The greatest difficulty, however, is to be sure that no irremediable condition has been inaugurated in the chronic cases, and in the acute cases just what progress toward such conditions will follow the subsidence of the acute attack. Only experience will tell this, and the swing of the pendulum in the last year or two, which is only now gaining full headway, brings the conviction that led Dr. Crile of Cleveland to state personally to me two years ago that he thought more gall bladders would have to be taken out in the future than had been the custom during the past few years.

Third, the possibility of the development of cancer in a diseased gall bladder, especially following cholelithiasis. In 95% of all cancers of the gall bladder stones are present. Then again, the number of cases of cancer of the gall bladder is variously quoted from 5% to 15%. But most important of all is the fact that those cases in which the cancer is discovered only after the gall bladder has been removed have remained in good health without recurrence or metastasis, while those large enough to be demonstrable at the time of operation have succumbed to the further progress of the disease.

It is not my purpose here to mention all the self-evident conditions requiring the removal of the gall bladder as mentioned in Deaver's[2] conservative article, but to strongly protest against the teaching that only when the gall bladder becomes functionless cholecystectomy should be done. I am firmly of the opinion, as Dr. Lilienthal, in 1904,[3] and again in 1911,[4] has ably concluded, "that, as a rule, any gall bladder which is worth operating on at all for biliary disease may with advantage be removed; that in the majority of cases this removal may be safely accomplished at the primary operation; that when for any reason this procedure appears dangerous, or otherwise undesirable we should look forward to completing the work at a subsequent sitting."

One word as to the technic of the operation. Contrary to the advice of most writers, I have found beginning at the fundus and working down, separating the gall bladder from its attachment to the liver, to be much simpler and easier than the more usual method employed of attempting to tie off the cystic artery first. I have never had any trouble from hemorrhage that a pad under a retractor in the hands of an assistant did not control. Usually such hemorrhage is negligible, especially if the separation is begun slightly to the gall bladder side. With the elevation of the patient into an exaggerated lordosis position after, not before, the abdomen is opened, the gall bladder is brought right up into the wound as the separation progresses, making ligation of the cystic artery and duct very simple and easy. My experience with this method has been so favorable that I have found no reason to change. You attack the parts nearest at hand and gradually bring the deeper structures within easy reach which satisfies the greatest principle of surgery—the surgery of common sense—i. e., seeing what you are doing, or bringing the enemy into the open as Dr. Gerster would put it.

One other point of technic is well worth mentioning. When you find that you must have more room for deeper work do not open downward as is one's first instinct to do, but upward in a curved direction following the border of the ribs which allows the liver to be rotated upward by traction and brings the region of the common duct much nearer to the surface.

References.

1. Journal of the American Medical Association, April 8, 1911.
2. American Journal of Medical Science, Vol. 135, p. 536.
3. Annals of Surgery, July, 1904.
4. N. Y. Medical Journal, July 1, 1911, p. 11.

Discussion.

Dr. J. Rosenstirn. I would not like to let this very interesting paper pass without discussion and

as you call upon me I will take the liberty of making a few remarks. Whilst I would extirpate every empyematous and every shrunken atrophic gall bladder, I would not do so with every bladder that simply contains gall stones. I believe in drainage of the gall bladder. It disinfects not only the gall bladder, but also the bile ducts and the larger biliary ducts of the liver, and often shows its usefulness in cholangitis. We know that infection ascends from the gall bladder into the liver and we know also that the primary cause of the formation of gallstones is an infection with such germs as colon bacilli, typhoid bacilli, etc. The objection against cholecystostomy on account of the possibility of a re-infection of the gall bladder may also be raised in cases of stones of the large bile ducts. We encounter the same conditions in stone of the hepatic, cystic or common ducts, and I doubt very much that Dr. Allen would advocate their extirpation. We drain the choledochus—we do not extirpate the choledochus, and in the many cases of choledochus stone I have operated I have never seen a bad effect from its drainage, and recoveries without recurrence have been the rule. I do not know what induced Dr. Allen to think that the belief claimed for cases of chronic pancreatitis, by drainage of the gall bladder, would show the pancreatic secretion in seepage from the drained bladder. The underlying idea is that pressure from the dilated bile ducts and liver, upon the pancreatic duct, will be relieved through drainage, and that thereby general improvement of the condition will ensue that will greatly relieve secretory retention in the pancreas, which has caused, or largely contributed to, the pathological condition of pancreatitis. With the relief of pressure the pancreatic fluid again is emptied into the duodenum and normal conditions are restored. We all know that chronic pancreatitis has often been mistaken for carcinoma of the pancreas. The gall bladder has been drained, although believing that the patient would die in a short time from the pancreatic disease, and later it has been found that the pathologic condition has subsided. The gall bladder is drained in such cases with good results.

Therefore, I believe that the pendulum will not swing in the direction of general extirpation of the gall bladder, as recommended in the first instance by its originator, Langenbuch, who advocated in the 80's of the last century the operation of cholecystectomy for every case of gall bladder disease. In cases of empyema of the gall bladder, it should be removed without doubt, but to demand this in every case of cholecystitis is, I think, unjustified, and will not, I believe, be generally accepted by the profession. The method of extirpating the gall bladder by commencing at the fundus is the old method as recommended by Langenbuch and also practiced by Kehrer, and I therefore cannot agree to its original recommendation being claimed for Dr. Lilienthal.

Dr. F. W. Birtch: I am convinced that in a large measure Dr. Allen's report contains the essence of biliary duct surgery as taught today. Gall bladder surgery has undergone many changes in the last few years. Not many years ago it was advocated to suture a tube in the gall bladder and then plaster the gall bladder tightly against the abdominal wall as a routine measure. This method gave way to suturing a tube in the gall bladder and then dropping the viscus back into its normal position. However, neither of these methods, as shown by subsequent case histories, completely relieves the patient. Surgeons all over the world came to realize that these methods were not yielding the desired results. This condition of affairs brought forth a flood of papers recommending cholecystectomy; but before we should recommend the routine removal of gall bladders we must ask ourselves: first, what is the function of the gall bladder? second, what metabolic influence the removal of this organ has on the individual, and third, what effect it has on digestion. Rost demonstrated that after cholecystectomy the bile first drops continuously into the duodenum but eventually there comes an intermittent emptying just as occurs when the gall bladder is intact. He also emphasized the fact that after cholecystectomy the biliary ducts may become much dilated, and that the stump of the cystic duct may become so large that it functionally serves as a new gall bladder. He explains the dilatation of the ducts on the ground that the sphincter of the papilla is strong enough to hold back the bile until it accumulates sufficiently to stretch the ducts. As far as metabolism is concerned, it is likely that the removal of the gall bladder has no effect on the individual. The loss of the gall bladder in many cases seems to disturb the digestion and this is probably due to the change in the manner in which the bile is emptied into the intestines. As far as the gross lesions of the gall bladder are concerned, such as hydrops of the gall bladder, empyema of the gall bladder, contracted gall bladder, perforated gall bladder, and gangrene of the gall bladder, most surgeons are of the opinion that the viscus should be removed.

Dr. Allen spoke of a small abscess which formed on the liver side of the cystic duct in his case. The cystic duct has very thin walls and perforations are much more common here than through the common duct. However, the cystic duct seldom perforates into the free abdominal cavity, but usually ruptures on the liver side as in Dr. Allen's case.

There are some interesting statistics on the mortality of biliary duct surgery which may not be out of place in this discussion. In simply opening the gall bladder the mortality is almost nil. When the gall bladder is drained in cases where the disease is still confined to the gall the mortality is about 1%. If the cystic duct is involved the mortality is about 2%. When the infection has extended into the common duct the mortality is 8%, varying from 3% in simple cases to 25% in obstructed cases. When malignant disease is present the mortality is as high as 70% or 80%.

The question of cholecystotomy or cholecystectomy is settled when the surgeon is able to recognize the pathology of the gall bladder at the operating table. If a gall bladder will return to normal, then drainage should be the operation of choice. If the gallbladder will not return to normal, then it should be removed. The point is that we must be able to recognize how badly the gall bladder is diseased in order to know how to treat it.

Dr. Allen, closing discussion: I do not want you to think that I remove every gall bladder. In regard to drainage the point wants to be considered that you might not need your drainage if you remove the infected gall bladder; your chief source of infection is gone. Then again, removal of the gall bladder does not prevent you from draining the common duct as long as it seems necessary.

The other point, that the disease is more serious as it progresses down from the gall bladder to the cystic and common duct, applies not to the operation, in my experience, but only to the character of and seriousness of the infection. The only deaths I have had were cases I have drained; like the one mentioned tonight that might have been alive today. Another death after drainage had an accompanying gangrene of the pancreas. Another had suppression of the urine on the second day. In a similar case recently, as to the age of the patient and seriousness of the infection, a cholecystectomy was done under local anesthesia; the woman's urine showed no signs of albumen the day before; slight on the day following; and on the same day, without any warning came down to ten ounces with the alarming signs of coma. If this

patient had been operated upon under ether I'm sure she would have been lost.

Of course, the main thing is the judgment of the surgeon as to the best operation for each case. But I think the argument that it is more serious to do cholecystectomy than cholecystostomy is wrong, especially when you consider the fact that when you remove the infected gall bladder you remove the main source of infection. You will find that both your immediate and your ultimate results will be better.

FISCHER'S THEORY OF EDEMA.

The recent publication of the second edition of Dr. Martin Fischer's work—and the trenchant criticism of Dr. Addis thereon—accentuates my interest in the subject, and if anything, strengthens the opinions I expressed in my review of the first edition written for this JOURNAL (November 1910, page 380).

No one who has repeated experimentally Fischer's work on the behavior of protein colloids can doubt the inevitable absorption and rejection of water under conditions frequently present in living bodies; but when we note the distribution of water in disease, its excessive presence in loose tissues of low specific gravity such as the subdermal, as against organs such as the liver, it seems as though mere weight of what is *assumed to be* protein colloid does not regulate the water absorbtion in an edematous area. Doubtless various reasons could be adduced in explanation but there is one possibility, which so far as I know has not been brought forward in this discussion which may indeed be the determining factor in this and other pathological conditions, namely, that the living protoplasm may be neither chemically protein or physically colloid. It is to be remembered that we know practically nothing of the static chemistry of life; all experimentation means chemical change and the appearance of non-living substance (Paraplasm). It is quite impossible to determine whether reactions so occurring are the product of the newly formed paraplasm or associated living protoplasm. Reactions whereby we determine that a given substance is a protein cannot be applied to the contents of a living cell and if they could, and positive reactions followed, we still would be unable to state that it was the living protoplasm and not the associated paraplasm that was reacting. This fact is implicit in Adami's designation of protoplasm as "proteidogenous matter." We must look on every living tissue, and cell, as made of protoplasm, and formed matter (ground substance in histology, paraplasm cytologically). On the occurrence of death the former becomes the latter and does so progressively in the act of dying.

I wish now to submit the thesis that the principles involved in Dr. Martin Fischer's theory probably apply only to the formed matter of cells and tissues. As all living tissues contain an abundance of such formed matter, in very varying amounts according to location and condition, we would still have abundant scope for these colloidal activities, without expecting them to be uniform in their incidence. It would explain why cellular tissues with the preponderance of ground substance are more prone to edema than muscles; why the lung is more affected than the liver; also why dying organs and cells with protoplasm passing into paraplasm increasingly absorb water.

While I advance this contention on theoretical grounds, I believe the problem is open to experimental proof, by direct observation of the behavior of living cells and tissues containing varying proportions of protoplasm and ground substance, when submitted to the influences inducing water absorption or rejection. Such work would be arduous, but almost certainly fruitful. I trust that some of my colleagues with more time and facilities for cytological research may undertake it, and I would gladly co-operate to the extent of my time and ability.

H. D'ARCY POWER.

PAN-AMERICAN MEDICAL CONGRESS.

Pursuant to an invitation by the President of the United States, authorized by act of Congress approved the third of March, 1915, the Seventh Pan-American Medical Congress convened in San Francisco on the seventeenth of June of this year. Owing to the short time between the authorization of the medical congress by the national Congress, a smaller attendance than was desired was realized, but in spite of the short time Argentina, Brazil, Cuba, Guatemala, Panama, Peru, Salvador and Venezuela were ably represented by delegates in person, and not a few who could not attend contributed to the support of the meeting by dues and by writing and submitting papers. The object of this special congress is, succinctly: (1) To promote personal and fraternal relations between the members of the medical profession of the Western Hemisphere; (2) To make the medical profession of each country more familiar with the educational, scientific and other medical resources of all the other American countries; (3) To consider problems of sanitation and public health administration of both national and international importance to the countries and colonies concerned; (4) To promote the development of periodical and other medical literature best calculated to promote the interchange of thought, as well as practical scientific co-operation by and between an All-American Medical Profession; (5) To cultivate the medical sciences.

In all of these the present meeting carried matters a certain distance forward. Certainly those Anglo-Americans who became well acquainted with Demaria of Argentina, Ramos and Roche Vaz of Brazil, Arteaga and Pons and Placeres of Cuba, Morales of Peru, Leiva of Salvador, Risquez of Venezuela, as well as those representatives of Guatemala who now, during the Exposition, are residents of San Francisco, feel that they have gained personal friends and have learned much of the status of medical education and of the hospital situations in the countries represented. The major proposition of the congress is, of course, the adequate consideration of national and international health problems, and in this the gain lay in the proceedings of the Section on Preventive Medicine and Public Health, under the Chair-